SOME PROBLEMS ASSOCIATED WITH SYSTEMS ANALYSIS

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INTRODUCTION

Broadly speaking, any orderly analytic study designed to help a decisionmaker identify a preferred course of action from among possible alternatives might be termed a systems analysis. As commonly used in the defense community, the phrase "systems analysis" refers to a formal study intended to advise a decisionmaker on the policy choices involved in such matters as base operations, weapon development, force posture, or the determination of planning objectives.

The idea of an analysis to provide advice is not new and, in concept, what must be done is simple and rather obvious. One looks at the entire problem in context and compares alternative choices in the light of their possible outcomes. Characteristically, such an analysis requires, in the most general view, three sorts of inquiry, any of which can modify the others as the work proceeds. There is a need, first of all, for a systematic investigation of the decisionmaker's objectives and of the relevant criteria for deciding among the alternatives that promise to achieve

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these objectives. Second, the alternatives need to be compared, usually in terms of their cost, effectiveness, timing, or risk—using a quantitative model insofar as it can be made representative of the situation. Finally, there must be an attempt to design better alternatives and select other goals if those examined are found wanting.

Even though the concept is simple, in practice the actual conditions of the analysis pose many problems. The fact that systems analysis still lacks an adequate theoretical foundation and is thus more an art than a science implies that it is pointless to expect success to come merely by following a set of definite rules. The environment, the circumstances under which the advice is offered, may compound the difficulties, as witness the decisionmaker who is in such haste for an answer that he actually makes it impossible for the analyst to conduct—or complete—the study properly. The analyst himself, after a period of fruitful work may have become so entrenched in his organization and so saturated with a decisionmaker's ideas that he has lost his independent viewpoint. The questions investigated almost always involve more than the efficient allocation of resources among known and accepted uses; often the objective or goals of the action to be taken must be determined first, and it may be more difficult to decide what ought to be done than how to do it. Indeed, the very aim of analysis itself, which is to suggest a course of action to someone else, tends to introduce all the difficulties and contradictions associated with value concepts, human behavior, and the communication of ideas.

I see these problems as two types: those inherent in all analysis of choice and those that are to some extent of our own making. Let me begin with a discussion of three problems of the first type: the selection of measures of effectiveness; the treatment of uncertainty; and the handling of considerations that cannot be readily quantified.

Later, I'll talk about problems that arise because we introduce bias, give insufficient attention to problem formulation, and sometimes forget the proper role of analysis.

MEASURES OF EFFECTIVENESS

In a 1966 report designed to accompany the bill authorizing funds for the Defense Department, the House Armed Services Committee remarked that "...the almost obsessional dedication to cost effectiveness raises the spector of a decision maker who ... knows the price of everything and the value of nothing."* This remark touches on the most serious problem in the application of systems analysis to national security problems—the determination of valid measures of effectiveness and the associated one of sensible criteria for choice.

Not only are national military objectives likely to be multiple, ill-defined, and conflicting, but the measures of their attainment are likely to be inadequate approximations at best. For indicating the attainment of such vaguely defined objectives as deterrence or victory, it is even hard to find measures that point in the right direction! Consider deterrence, for instance. It exists only in the mind—and in the enemy's mind at that. We cannot, therefore, use some "scale of deterrence" to measure the effectiveness of alternatives we hope will lead to deterrence, for there is no such scale. Instead, we must use such approximations as the potential mortalities that might be suffered if war were to come. Consequently, even if a comparison of two systems indicated that one could inflict 50 percent more casualties on the enemy than the other, we still could not conclude that this meant the system supplies 50 percent more deterrence. In fact, since in some circumstances it may be important not to look too threatening, we might even

*House Rept. 1536 Authorizing Defense Procurement and Research and Development, 16 May 1966.

argue that the system capable of inflicting the greatest number of casualties may provide the least deterrence!

The inability to determine good measures of effectiveness is a severe limitation on the usefulness of analysis. Suppose we are seeking to determine the characteristics of future tactical aircraft. If a single measure such as weight of ordinance delivered is used, the aircraft are likely to look like the B-52 or the C5. Some combination is required that takes account of all the characteristics that are important to the effectiveness of such aircraft —qualities such as speed, bombing accuracy, runway length necessary for takeoff, maintainability, ferry range, and so forth. Possibilities to use in a comparison of different types of tactical aircraft are something like the change in the outcome of the projected ground battle or the very popular notion of the forward edge of the battle area (the FEBA). Ultimately, an entire profile of measures may be required to give a criterion for choice. One recent proposal for a study of the mix of the tactical air forces in Europe suggests using the "history" of a range of hypothetical wars in which NATO is the defender, comparing the effectiveness of the various force mixes in resisting an overwhelming attack on the basis of time indices—such as the time it takes the enemy to reach the Weser, then the Ems, then the Rhine, then Paris; the time it takes our side to reach air parity, then local superiority, then general superiority, then air supremacy. There are fifteen indices in total, the others involving losses and force ratios.

How does one go about finding a satisfactory measure of effectiveness? It is essentially an art; a matter of trial and error and good judgment. It may be hopelessly impossible to think out a good measure in advance. The practical way may be to take a rather crude value scale, see what result its use leads to, and then, if that result

is not in accordance with common sense, revise it. For instance, consider the slightly apocryphal example of the analyst who set out to use a linear programming model to evolve an optimal reducing diet. He decided that an ideal measure of effectiveness would be to get the most volume within the constraints of the necessary nutrients and calories. Well, he put his model on the machine, ground away, and came up with watermelon. That showed him he had better consider dried weight to exclude water. When this was done he came up with bouillon cubes, which consist largely of salt. Since this would not be very palatable and might be injurious to health, he made still another choice, and continuing in this way, he finally evolved a satisfactory rule of choice.

THE TREATMENT OF UNCERTAINTY

Systems analysis is concerned with problems whose essence is uncertainty. While it is possible to forecast events in the sense of mapping out possible futures, there is no way to predict a single future for which we can work out the best system or determine an optimum policy. Consequently, we must consider a range of possible futures or contingencies. In any one of these we may be able to designate a preferred course of action, but we have no way to determine such an action for the entire range of possibilities. As a result, defense planning is rich in the kind of analysis that tells what damage could be done to the United States given a particular enemy force structure; but it is poor in the kinds of analyses that evaluate how we will actually stand in relation to our possible enemies in years to come.

It is not enough simply to acknowledge that uncertainties exist and to warn that some things have been left out of the analysis because of a lack of information, for such issues may have a critical effect on the conclusions. The

user has to come to grips with these issues and he needs to know what their effects must be, how likely they are, when he can expect them, and what he can do about them.

Uncertainties whose probability of occurrence is more or less objective or calculable can be handled in the model by Monte Carlo or other methods. The treatment of such uncertainty is a considerable practical problem, however, and a challenge to the analyst. The pitfall for model-builders lies in accepting this challenge—to the neglect of the real or unforeseeable uncertainties. These typically involve forms of ignorance that cannot be reduced to probabilities, and their consequences can be devastating.* The objective in system studies is not to learn what can happen in a given situation with a specific probability as the consequence of chance fluctuation, but to design a system or determine a policy so that any fluctuations are unimportant.

What can we do in the face of uncertainty? One possibility is to buy time, to delay decision until more information is available. A second is to buy information, to alleviate our uneasiness by data collection. Another is to buy flexibility as a hedge, for example, by developing more than one alternative. Sometimes one can eliminate alternatives by a "dominance" or _a fortiori_ argument. Suppose that in a particular planning decision experienced judgment strongly favors alternative A, but the analyst feels that alternative B might be preferred. In performing the analysis, one may choose deliberately to resolve the major uncertainties in favor of A and then see how B compares under these adverse conditions. If B still looks good, the case in favor of B may be very strong.

In addition, sensitivity analysis and contingency analysis must always be carried out. Let me illustrate in

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*Particularly those uncertainties that depend on human caprice. As one of Damon Runyan's characters (I think) put it: "Nothing what depends on humans is worth odds of more than 8 to 3."

terms of a simple hypothetical and unrealistic example from agriculture.

> A farmer must decide what crop or crops to plant without knowing whether the weather will be wet, moderate or dry. An analysis is performed to help him decide. A popular approach is employed in which the analysis is repeated in turn for each of the three distinguishable types of weather, in each case determining the best crop to plant for that type of weather. Considering all possible crops, it is found that for wet weather corn would be best, for moderate weather oats would be best, and for dry weather wheat would be best. The principal results presented to the farmer consist of the findings concerning the best crops in the three types of weather, the best yields achievable in each contingency (i.e., the wet-weather yield of corn, the moderate-weather yield of oats, and the dry-weather yield of wheat), and estimates of the probabilities of wet, moderate and dry weather. The implication is that the farmer ought to make his choice from the "preferred" crops, corn, oats or wheat, or perhaps a combination of these to provide some all-weather insurance.
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> The farmer is not satisfied with the analysis, however. He points out that the analysis tells him what crop he should plant if he knew for certain what the weather would be, but he doesn't see how this helps him to decide what to plant when he doesn't know what the weather will be, except for the weather probabilities. He would like to know, for example, what will happen if he plants corn and the weather turns out moderate or dry, and similarly for the other crops. The analyst therefore prepares a two-way contingency table, showing for each of the three "preferred" crops the yields in wet, moderate and dry weather. Yields for various mixtures of these crops are also shown in the various types of weather. It is found that each of the three crops is rather narrowly tailored for that type of weather in which it is best, and gives disastrously poor yields in other types of weather. Oats, for example, gives very poor yields in wet or dry weather, but very good yields in moderate weather. The farmer can insure against disaster by planting a mixture of corn, oats and wheat, thereby obtaining a fair overall yield whatever the weather.

The farmer is still not satisfied, however. The contingency table does give him the information he wants on the three "preferred" crops, but he would like to see the same information for some other crops, even though they have been ruled out as "inferior" in the analytical optimization. The analyst obligingly expands the contingency table to show the yields of various other "inferior" crops in wet, moderate and dry weather. At this point it is noted that cane, which is inferior to corn in wet weather, inferior to oats in moderate weather, and inferior to wheat in dry weather, gives a "pretty good" yield in all types of weather, providing better all-weather insurance than can be achieved with any combination of the three "preferred" crops. This particular farmer, having a pronounced aversion to risk, decides that of all the crops he prefers the weather-yield pattern of cane over that of any other crop or combination of crops. Another farmer, looking at the same table, might prefer to take somewhat more of a chance on alfalfa, another "inferior" crop shown to give a rather good yield in wet or moderate weather but a poor yield in dry weather. Still another might prefer to take a greater chance on corn, but not necessarily because it was one of the "preferred" crops in the original analysis.

The first approach in the example above, which narrows down the alternative systems to those which are "preferred" for each of the various situations or sets of assumptions about the uncertain factors, is far from uncommon in actual applications of systems analysis. It can be useful in indicating some of the systems which merit consideration by the planner or decisionmaker. It can be worse than useless, however, if it leads the customer to limit his attention only to these "preferred" systems. In the example above, the farmer was not so misled; in practice, however, there is a real danger that the customer will be misled into confining his choice to an unduly narrow menu of alternatives.

The approach evolved toward the end of the example above has the advantage of not ruling out systems which the customer ought to consider. It has the disadvantage, however, of not ruling out very many systems at all; it eliminates only those "inefficient" systems which are inferior or at most equal to other systems in <u>all</u> situations or for <u>all</u> assumptions about uncertain factors. In <u>many</u> cases the remaining

"efficient" systems are too numerous to present to the customer; some means of narrowing them down further to a manageably small list must be found if the analysis is to be useful to the customer. This may require going beyond the bounds of strictly legitimate analysis, by such expedients as optimizing on the basis of weighted values of performance in the various situations or for the various assumptions about the uncertain factors, or eliminating systems by the direct application of the analyst's judgment. The analyst cannot avoid employing judgment in initially formulating the analysis and in initially limiting consideration to some subset of systems out of the infinity of all conceivable systems; further employments of judgment later in the analysis may also be unavoidable.

Whatever approach is used in narrowing down the list of systems to be presented to the customer, the approach should be described as explicitly as possible. The presentation should include, among other things, a contingency table, showing for each system its performance and cost in each of the various relevant situations and/or for each set of assumptions about the uncertain factors. Digesting this information and using it in making decisions or plans puts a heavy burden on the decisionmaker or planner, but it can't be helped. Systems analysis does not relieve the customer of the responsibility for facing the uncertain consequences of his decisions or plans; it can, however, help him face uncertainty with a better appreciation of the relevant considerations than he might otherwise have had.*

HANDLING THE NONQUANTIFIABLES

One obvious limitation is that for broad questions the analysis is necessarily incomplete. Time and money place sharp limits on how far any inquiry can be carried. Almost all systems analyses must stop far, far short of completion either for lack of funds, of time, or of justification for spending further funds or time on them.

Still more important, however, is the fact that even with no limitations of time and money analysis can never treat all the considerations that may be relevant. Some are

*Donald M. Fort, Systems Analysis as an Aid in Air Transportation Planning, The RAND Corporation, P-3293-1, March 1966.

too intangible—for example, how some unilateral U.S. action will affect NATO solidarity, or whether Congress will accept economies that disrupt cherished institutions such as the National Guard or radically change the pattern of domestic spending. Considerations of this type can play as important a role in the choice of, for example, alternative military policies as any idealized cost--effectiveness calculations. But ways to measure these considerations even approximately do not exist today, and they must be handled intuitively. Other considerations involve moral judgments—for example, whether national security is better served by an increase in the budget for defense or for welfare, or under what circumstances the preservation of an immediate advantage is worth the compromise of fundamental principles. The analyst can, at the very least, apply his own judgment and intuition to these considerations, thus insuring that they are brought to the attention of the man with the responsibility for the decision. The problem is to do something more.

When considerations of this type dominate and we cannot adequately approximate the situation on a computer or by mathematical equations, a different approach is required, one that directly involves experts whose knowledge should be of help in attacking intangible and moral considerations.

Ordinarily, we would like to have the advice of several experts. The traditional way to try for their consensus has been to assemble them as a committee, to let them discuss the problem freely, and to require that they arrive at a joint answer. Committees, however, often fail to make their assumptions and reasoning explicit, since their findings may be obtained through bargaining. Better judgments can be obtained by asking the experts to work in a group exercise, which might range from a simple structured discussion to an "operational game."

This latter approach is a form of simulation involving

role playing by human subjects. Here the game structure —that is the model—furnishes the participants with an artificial, simulated environment within which they can jointly and simultaneously experiment, acquiring through feedback the insights necessary to make successful predictions within the gaming context and thus indirectly about the real world. For example, to study the economy of an underdeveloped country, the various sections of the economy might be simulated by specialized experts. These experts or specialists would be expected, in acting out their roles, to use their intuition as experts to simulate as best they could the attitudes and consequent decisions of their real-life counterparts. For instance, a player simulating a goods-producing sector of the economy might, within constraints, shut down or expand manufacturing facilities, modernize, change raw material and labor inputs, vary prices, and so on. There would also need to be government players, who could introduce new fiscal or monetary policies and regulations (affecting taxes, subsidies, tariffs, price ceilings, etc.), as well as social and political innovations with only indirect economic implications (social security, education, appeals to patriotism, universal military service, etc.). In addition, there would have to be a control team to lay down the rules governing the players' options and constraints and to evaluate the actions taken within these rules.

An important need in all analysis is to set the context. One approach is what military systems analysts call "scenario writing." This is an effort to show how a future state of affairs might evolve out of a present one. A scenario is thus a primitive model that helps define the relevant environments or situations in which to test the alternatives. A collection of scenarios provides an insight into how future trends can depend on factors under our control and suggests policy options to us.

Another type of group action that can play the role of the model when we can't build a satisfactory one, somewhat less structured than the operational game, attempts to improve the panel or committee approach by subjecting the views of individual experts to each other's criticism without actual confrontation and its possible psychological shortcomings. In this approach, called the Delphi method by its principal contributor Olaf Helmer, direct debate is replaced by the interchange of information and opinion through a carefully designed sequence of questionnaires. At each successive interrogation, the participants are given new and refined information in the form of opinion feedback, which is derived by a computed consensus from the earlier part of the program. The process continues until either a consensus is determined, or the conflicting views are documented fully.*

In addition to the problems inherent in all policy analysis, there are additional problems we create for ourselves. Among others, these have to do with introducing bias; attacking the wrong problem; and forgetting the proper role of the analysis.

BIAS

The most serious error in analysis is to look at an unduly restricted range of alternatives. Although narrowing the range of choice certainly makes the evaluation easier, we may pay a high price if some of the excluded alternatives are better than those remaining.

The most frequent cause of failure to look at the full range of alternatives is an "attention bias." This often takes the form of an unconscious adherence to a "party line" or "cherished belief." All organizations foster one to some extent; the military services, the civilian agencies of the government, and private businesses are no exception. Experience suggests that Herman Kahn was right when he called the party line "the most important single reason for the

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*For additional discussion, see O. Helmer, Social Technology, The RAND Corporation, P-3063, February 1965.

tremendous miscalculations that are made in foreseeing and preparing for technical advances or changes in the strategic situation."* Examples are plentiful: the general whose gaze is so fixed on "winning" a local war militarily that he excludes other considerations; or the statesman so convinced that peace can be maintained through deterrence that he completely disregards what might happen should deterrence fail; or the weaponeer who is so fascinated by startling new weapons that he assumes they can of course be used.

The history of strategic bombing studies since World War II illustrates the workings of this influence. In World War II, the bombing problem was to penetrate the defenses, bomb accurately, and return. The bomber's concern was with enemy fighters, antiaircraft guns, and missiles—not with enemy bombers. For years, even long after the Soviets were expected to have nuclear weapons, studies paid no serious attention to the possibility that our bombers might be vulnerable on the ground, and did not consider an attack on the enemy's grounded bombers as a strategy. Requirements and specifications for future bombers hardly considered the problem of surviving the enemy offense. This was not stupidity on the part of anyone. For instance, in the Navy-SAC controversy over the B-36 in 1949, the Navy challenged the B-36 on every basis it could think of—including the argument that strategic bombing was immoral—but the question of its vulnerability on the ground did not come up. RAND strategic bombing studies, even after the seriousness of the problem of bomber survival on the ground had been pointed out, continued to concentrate on such questions as speed, altitude, low versus high penetration, supersonic dash, bombing altitude, small versus large planes, and what targets to select. Thus, in a 1952 comparison of aircraft with missiles for strategic bombing we ignored any effect on the comparison of possible differences in ground survivability—and no one we briefed

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*H. Kahn and I. Mann, Ten Common Pitfalls, The RAND Corporation, RM-1937, July 1957, p. 42.

took us to task for this omission! The Russians and the British took even longer than we did to recognize ground vulnerability, and some people think the French have not yet really absorbed the idea. It took the extensive briefings of a RAND study devoted to proving that base vulnerability was a serious threat to national security to get major attention on the problem.

A party line can be most influential in shaping the study during the early stages. What can happen is that the participants and successive reviewers become aware that some of the alternatives or certain assumptions being considered are frowned on by higher ranking officers. It seems useless, even hazardous, to support such unpopular views strongly and gradually they may be stressed less and less or even forgotten. In its most extreme form, this influence can, in effect, cause the analyst to lose his independence of view.

A similar pitfall is to expect a man or the organization that created a system to discover its faults. Either may fail completely to take into account some technical notion or fact. An analyst is sometimes shocked to discover that other organizations regard the system he is investigating as controversial at best, or that they even consider his own people to be completely wrong. For example, he may find out that simple countermeasures can be devised to render the system almost worthless. As an example, consider the English invention of "window" or, as we call it here, "chaff." In 1937, in Great Britain, the merits of radar and infrared were being argued. Talk about shutting down research on infrared because of its vulnerability to countermeasures started. It was then that R. V. Jones, a physicist who was working on infrared, came up with the idea of "window" to show (in his words) "They weren't so invulnerable themselves." Watson-Watt, the English radar pioneer, apparently never had quite the heart to urge trials for

Jones' device, which seemed so certain to spoil his "beautiful" invention. Sometime later, others had the trials carried out.

The first step, and the most effective one, in eliminating bias is to recognize that it is likely to exist. The second is to have the analysis critiqued by people with a different set of cherished beliefs or party line. To appraise a system and discover such things, good analysts obligate themselves to consult people with an adverse opinion of the worth of the system, largely because they know how hard it is to get a scientist or engineer to display much ingenuity in tearing down a technically brilliant design that he has been working on for years.

Another manifestation of parochialism in analysis occurs when service A proposes a future weapon system with fabulous properties for victory and compares it with the semiobsolete current equipment of service B rather than with the new system that B hopes to introduce, or uses a "scenario" ideally suited for making the good points of A apparent.

PROBLEM FORMULATION

It is all-important to tackle the "right" problem. A large part of the investigators' effort must be invested in thinking about the problem, exploring its proper breadth, and trying to discover the appropriate objectives and to search out good criteria for choice. If we have not chosen the right set of alternatives to compare, we will not discover the best. But if we have chosen the wrong objective, then we might find a solution to the wrong problem. Getting an accurate answer to the wrong question is likely to be far less helpful than an incomplete answer to the right question.

A serious pitfall is the failure to allocate the total time deliberately so that a sufficient share of it will be

spent in deciding what the problem really is. It is a ptifall to give in to the tendency to "get started" without having devoted a lot of thought to the problem.

In the first systems analysis I worked on I fell into this pitfall. The analysis was being carried out to help in the design of a strategic bomber for the mid 1950s. One moot question was whether or not to put a tail turret with machine guns on the bomber. The day I reported for work it was suggested that I try to determine how the probability of hitting an attacking fighter was affected by the correlation between aiming points in air-to-air machine gun fire—something always present because the shots are not aimed independently. I saw an interesting mathematical problem, and started right to work. Several months later, after we had found a satisfactory approximation and checked it by a Monte Carlo simulation, I saw some test firings of an early version of an air-to-air guided rocket. I was much impressed. Not until some time later, however, did it suddenly dawn on me that by the time the bombers we had under study could become operational, they would not have an opportunity to fire back at fighters with machine guns. Thinking about the problem as a whole and asking a few questions would very likely have saved all of us a lot of time.

Systems studies have resulted in rather important changes, not only in how the policymaker carries out his activity, but in the objectives themselves; hence it would be self-defeating to accept without inquiry the sponsors' view of what the problem is. But how is the analyst to know that his formulation of the problem is superior? His only possible advantage lies in analysis. That is, the process of problem formulation itself has to be the subject of analysis. What this means is that, using the few facts and relationships that are known at this early stage and assuming others, the analyst must simply make an

attempt to solve the problem. It is this attempt that will give him a basis for better formulation.

Let me offer an example. For fiscal year 1952, Congress authorized approximately $3.5 billion for air base construction, about half to be spent overseas. RAND was asked to suggest ways to acquire, construct, and maintain air bases in foreign countries at minimum cost. The analyst who took on this problem accepted it as given as essentially one of logistics. He spent a long time—several months, in fact—thinking about it before he organized a study team. Although he had little of the information needed to make recommendations, he began to look at the problem in relation to the Air Force as a whole. He argued that base choice would critically affect the composition, destructive power, and cost of the entire strategic force and thus that it was not wise to rest a decision on base structure and location merely on economy in base cost alone. He came to the conclusion that the real problem was not one of the logistics of foreign air bases, but the much broader one of where and how to base the Strategic Air Force and how to operate it in conjunction with the base system chosen. His views prevailed, and he led the broader study.* An Air Force Committee later estimated that the study recommendations saved over $1 billion just in construction costs. In addition, it sparked a tremendous improvement in strategic capability, particularly with regard to survival.

In analysis, the problem never remains static. Interplay between a growing understanding of what it involves now and might involve in the future forces a constant redefinition. Thus, the study just mentioned, originally conceived as an exercise to reduce costs, became in the end a study of U.S. strategic deterrent policy. Its first

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*For the full report, see A. J. Wohlstetter, F. S. Hoffman, R. J. Lutz, and H. S. Rowen, Selection and Use of Strategic Air Bases, The RAND Corporation, R-266, April 1954.

recommendation was to reduce SAC vulnerability by cutting sharply the number of functions performed at overseas bases and thus the time during which the force was exposed on these bases. That this also reduced costs was secondary.

THE ROLE OF ANALYSIS

The role of systems analysis is to sharpen the intuition and judgment of the decisionmakers; to provide advice concerning a particular question. Problems arise if we forget these things.

It is a dangerous pitfall to attempt to set up a model that treats every aspect of a complex problem simultaneously. What can happen is that the analyst finds himself criticized because the first model he has selected has left out various facets of the situation being investigated. He is vulnerable to these criticisms if he doesn't realize that the question being asked, as well as the process being represented must determine the model. Without attention to the question, he has no rule for guidance as to what to accept or reject; he has no real goals in view and no way to decide what is important and relevant. He can answer criticism only by making the model bigger and more complicated. This may not stop the criticisms, for something must always be left out. The size of the model is then determined not by what is really relevant but by the capacity of the computing machine.

Of course, it is easy for the analyst to become more interested in the model than in the problem itself. Technical people with specific training, knowledge, and capability like to use their talents to the utmost. It is easy for analysts to focus attention on the mechanics of the computation or on the technical relationships in the model rather than on the important questions raised in the study. They may thus find out a great deal about the inferences that can be drawn from the model, but very little about the question they set out to answer. But the role of the

analyst is to provide advice, not to build new tools or to advance the art of operations research.

A serious pitfall is to believe that, if the work is done correctly, systems analysis is unlimited in the advice it can supply a decisionmaker. This is far from the case. No model can substitute for the decisionmaker—the limitations on analysis mentioned earlier prevent this. However, it has been proposed a number of times (even to the extent of writing a study contract) that a general computer model for strategic war be set up to supply weapon designers with a systematic evaluation of their design concepts and to enable the Department of Defense to evaluate the worth of alternative "design solutions" developed by competing contractors.

One argument for such a model notes that "the choice of assumptions, the forecast of the future, and the methods of analysis have a marked influence on the performance and physical characteristics of the weapon system set forth as preferred or optimal"; therefore, a uniform framework would mean that "the results obtained by the various contractors would be comparable since the effect due to variation in the assumptions they might have chosen to form their models would have been eliminated." This may indeed be the case, but will the end result be desirable? A rigidly specified framework may mitigate one sort of undesirable bias—by making it difficult for an analysis to be used to rationalize conclusions already otherwise derived—but only at the severe risk of introducing other biases.

A fundamental objection is that a uniform framework necessarily conceals or removes by assumption many extremely important uncertainties, and therefore tends to lead to solutions that disregard the value of hedging against those uncertainties. Another is that even if efforts were made to keep the model up to date, this would turn out to be

impossible, for the analyst must be able to modify his model in the terminal stages of his study to accommodate information acquired during the early phases. Indeed, in a problem involving the struggle between nations, there are so many factors of shifting importance, and such radical changes in objectives and tactics are likely, that most models are obsolete long before the recommendations from the study can become accepted policy. Moreover, if a model or a mathematical formula were used to indicate which proposal to select, the proposers' emphasis would soon focus on how to make his design look good in terms of this analytic definition, not on how to make it look good against the enemy—a much harder problem.

CONCLUDING REMARKS

How can systems analysis provide better advice than other more traditional ways to furnish advice—say, by an expert or a committee? In areas such as national defense planning, where there is no accepted theoretical foundation, advice obtained from experts working individually or as a committee must of course depend largely on judgment and intuition. So must the advice from systems analysis. But the virtue of systems analysis is that it permits the judgment and intuition of the experts in relevant fields to be combined systematically and efficiently. The essence of the method is to construct and operate within a "model," a simplified, stylized representation of the real situation appropriate to the question. Such a model, which may take such varied forms as a set of mathematical equations, a computer simulation, an operational game, or even a purely verbal description of the situation in the form of a "scenario," introduces a precise structure and terminology that serve primarily as an effective means of communication, enabling the participants in the study to exercise their judgment and intuition in a well-defined context and

in proper relation to the judgment and intuition of others. Moreover, through feedback from the model (the results of computation, the countermoves in the game, or the critique of the scenario), the experts have a chance to revise early judgments and thus arrive at a clearer understanding of the problem and its context, and perhaps of their subject matter.

As Charles Hitch put it: "Systems analysis should be looked upon . . . as a framework which permits the judgment of experts in numerous subfields to be utilized—to yield results which transcend any individual judgment. This is its aim and opportunity."*

The process has problems and limitations, but the alternatives to analysis have the same and even greater ones.

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*Analysis for Military Decisions, E. S. Quade, Editor, Rand McNally and Co., Chicago, 1964, p. 23.